



DEPARTMENT OF HEALTH, EDUCATION, AND WELFARE
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May 7, 1966

Dr. Francis H. C. Crick
Laboratory of Molecular Biology
University Postgraduate Medical School
Mills Road, Cambridge, ENGLAND

Dear Francis:

Your interpretation of our PNAS paper is completely, absolutely wrong - wrong in every respect - and thoroughly unjustified. I can tell you unequivocally that you have misinterpreted the paper and my actions and have attributed thoughts and motives to me which I have never had.

I will try to answer in detail every point you have raised in your letter but the concise facts of the situation are that I have accumulated a very large backlog of unpublished data, for I almost always prefer to concentrate on current work rather than prepare manuscripts. Such long delays have upset people in my lab, and I have tried to get these papers out first. This was the case with the Kellogg PNAS paper. It was written only after I returned from La Jolla in February. ~~It~~ It is ridiculous for you to even consider the possibility that the PNAS paper represented an attempt to gain some kind of priority. If priority had been any concern to me I would have published our findings with Holley's sRNA last summer. This manuscript and a dozen others has been on my desk since summer.

Now, regarding your questions: First, the statement that I deliberately withheld information from you when you asked whether I had published the binding studies I presented at the Gordon Conference. No other manuscript on sRNA fractions was either in press or in preparation. We have been working intermittently on sRNA fractions with B. P. Doctor, for at least one and one-half years, but the work had gone very slowly due to errors and lack of triplets, and at that time I had not even begun to think about the preparation of a manuscript. I did not present these data at the Gordon Conference and did not think we were even near publication. Certainly, no attempt whatsoever has been made to conceal these studies. I think I told you of them earlier, and I know that Khorana's laboratory knew of them, for only a few weeks ago at the Federation meeting in Atlantic City, Dieter Söll asked about our studies on Ser-sRNA fractions which I had described to him at the last Gordon Conference. Last November, Kellogg saw Marcker, described the results of our sRNA fraction studies and also the experiments which we hoped to do. Marcker visited our laboratory in December and we brought him up to date on these experiments. We had obtained Met-sRNA fractions to study the mechanism of initiation and termination, and told Marcker that we would report the Met-sRNA results only as a confirmation of his and Brian Clark's findings. After their paper was circulated in the IEG, we thought it proper to report our results, and we emphasized in the PNAS paper that the results represented

a confirmation of Clark and Marcker's findings.

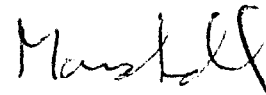
Just before leaving for La Jolla in February Kellogg told me that he and Doctor were quite concerned because no publication has resulted from such a long collaboration. I asked him to organize the data and prepare an outline of a manuscript so that I could assess it after I returned from La Jolla. I reviewed all of the data after I returned and went over a rough draft of a manuscript. The data looked better than I had expected. Kellogg worked very intensively, and I helped him, and the manuscript was ready for publication in another week. I expected to contact you, Brian and Gobind to obtain permission to cite your IEG manuscripts and work in preparation. The next day I injured a vertebral disc or something and was out of the lab for the next two weeks. Many necessary things were left undone during this period. However, I contacted Gobind from my home by telephone, told him about the paper, and he gave me his permission to cite his work. You are incorrect in saying that I did not tell Gobind about the paper. The letters to you and Brian should have been sent but were not and I apologize for this. You called me at home from Ithaca about this time. It was a perfect opportunity to ask you for permission to cite your paper but it just did not occur to me while we were talking. I very much wish that it had. Do you really think that anyone would have planned a subterfuge of this sort, contacted Gobind to obtain permission, and then would not obtain permission from you? It just doesn't make sense.

Now, for the motive which you ascribe to me. It is just incredible that you should even think that the PNAS paper represents an attempt to gain some sort of priority. If I had been concerned with priority I most assuredly would have published our studies with Holley's sRNA last summer. Philip Leder and I finished the experimental work on Holley's sRNA before the Gordon Conference and the manuscript was written by Philip in August just before he left. Leder has been at the Weizmann Institute in Israel since then, and you can easily verify this by contacting him. The Kellogg PNAS paper is absolutely trivial compared to the Holley Ala-sRNA study, for the latter demonstrates alternate codon recognition by sRNA of known sequence and suggests a mechanism of codon recognition as well as the base sequence of the anticodon. Again I repeat, if I had been concerned about priority I most assuredly would have published the yeast Ala-sRNA studies last summer. I am at a complete loss to understand what type of priority could be established by the PNAS paper, and why you consider it to be anything more than the relatively mundane paper that it is. How can you possibly think that anyone would devise an elaborate, underhanded scheme for a paper such as the PNAS one, and allow a truly good paper to remain unpublished since summer. And most of all, rather than being concerned about personal priority I have been concerned about my negligence in not getting it off to J. Mol. Biol. I most certainly had no intention whatsoever of delaying publication of your papers by suggesting that our manuscript accompany yours. When you told me that you and Gobind were simultaneously submitting papers to J. Mol. Biol. it seemed like a very natural thing to suggest that our paper accompany yours. However, I have a large backlog of partially finished manuscripts which should have been sent out a long time ago, and people in the laboratory who have worked on these papers have very rightfully been upset by the too great delay in publication.

Two extremely overdue, but relatively unimportant manuscripts were sent recently to J. Mol. Biol., but the manuscript on Holley's sRNA remains unfinished. I have been absorbed in current work in the laboratory and tried to continue this work and simultaneously write manuscripts. I write much too slowly, so it hasn't worked out at all.

I can only say that angry as I have been at your interpretations, I am glad that you told me what you were thinking. Although I can see many ways in which this misunderstanding could have been prevented, common sense alone should have told you that your interpretations were appallingly unjustified.

Sincerely,

A handwritten signature in dark ink, appearing to read "Marshall", with a stylized flourish at the end.

Marshall W. Nirenberg